Symbolic Interaction

Diagnosis as Topic and as Resource: Reflections on the Epistemology and Ontology of Disease in Medical Sociology

Darin Weinberg
University of Cambridge, United Kingdom

This article notes an enduring ambivalence in medical sociology concerning the epistemology and ontology of disease and shows this is precisely an ambivalence concerning whether biomedical disease categories are best understood as topics of, or as resources for, medical sociological research. The first section critically reviews the topic/resource debate in ethnomethodology. The second section elaborates upon the pertinence of this debate to sociological debates directly concerned with the epistemology and ontology of disease. The article concludes by demonstrating how framing the epistemology and ontology of disease in terms of the topics and resources of medical sociological analysis serves to clarify the work of thinking sociologically about disease and helps overcome protracted theoretical challenges that have persistently troubled medical sociological research.
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Medical sociologists have long debated whether to be accomplices or critics—insiders or outsiders—to the health care professions (Straus 1957). Nowhere has this been more fundamentally addressed than in discussions of the epistemology and ontology of disease. Since the nineteenth century mainstream biomedicine has itself equated the ontology of disease with the anatomical and physiological pathologies of the body that it has itself scientifically identified. By these orthodox biomedical lights, the disease categories devised by biomedical scientists are assumed to faithfully mirror the objective and mind-independent realities underlying our various subjective experiences of sickness (cf. Jutel 2011; Rosenberg 2002). The work of biomedical diagnosis is here understood as epistemologically insulated from the noisiness, social biases, and equivocality that often trouble interpretation in the broader culture by a

Direct all correspondence to Darin Weinberg, Department of Sociology, University of Cambridge, 16 Mill Lane, Cambridge CB2 1SB, United Kingdom; *e-mail*: dtw23@cam.ac.uk.

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strict adherence to the rules of scientific method. And, precisely because diagnosis has long been construed as a strictly value-neutral and scientific enterprise, it was for most of the twentieth century considered generally exempt from sociological scrutiny. The processes through which disease categories are formulated and put to work were uncritically assumed to systematically transcend the cultural limitations of ordinary social interaction — taking the form of universal and value-free scientific procedures — and to that extent immune to the explanatory repertoire of sociology.

Hence, for example, when Talcott Parsons in 1951 formulated the sick role, the doctor's role was conceived as purely technical, informed only by her medical scientific training and confined to the scientific diagnosis and management of disease and/or disability. When sociologists began to study illness behavior and illness experience, they too took for granted that the primary independent variable was the biomedically identified disease or disability itself, albeit, of course, supplemented by a range of social variables (cf. Conrad 1987; Mechanic 1962; Pierret 2003; Strauss and Glaser 1975). But this relatively uncritical deference to biomedical knowledge was not to last. Eliot Freidson (1970) was among the first to interpret medical claims to possess an epistemologically privileged knowledge of disease as little more than efforts to legitimate medicine's social power. Practitioners of what Michael Bury (1986:140) has called post-Freidsonian medical sociology — that is, symbolic interactionists, feminists, Foucauldians, ethnomethodologists, Marxists, and other social constructionists — have elaborated extensively upon this theme. In stark contrast to earlier work, this research often questions the ontological validity of medical categories, suggesting many conditions understood as diseases or disabilities are in fact socially manufactured primarily for sociologically rather than biomedically discoverable reasons. With the advent of the influential medicalization literature, social constructionists began providing sociological rather than biomedical explanations for why and how it was that various conditions including bizarre behavior, homosexuality, alcohol and drug use, over-eating, under-eating, infertility, aging, childbirth, child development, youthful boisterousness, sadness, stupidity, and many others came to be defined as specifically medical problems (cf. Busfield 2017; Conrad 2007; Conrad and Schneider 1992). Likewise, the social model of disabilities literature has also influentially highlighted the myriad social, rather than biomedical, challenges suffered by those with putative disabilities (cf. Oliver 1990; Shakespeare 2006).

By insisting the biomedical diagnosis of disease and disability are not innocent of social values, interests, and influences, social constructionism radicalized the explanatory promise of medical sociology. More fundamentally, biology — once understood as the universal bedrock upon which the multitude of human cultures is built — was recast as diverse forms of socially orchestrated *work* accomplished across a range of historically evolving social institutions (cf. Fleck 1979; Fujimura 1992; Haraway 1991; Mol 2002). While indisputably invigorating for many medical sociologists, these initiatives made many others decidedly uncomfortable. Critics of social constructionism contended that although there are certainly the odd cases of medical over-reach, surely it was absurd to suggest conditions like cancer, diabetes, or heart

disease are "mere" social constructions (cf. Bury 1986; Shakespeare 2006; Timmermans and Haas 2008; Turner 2004; Williams 2006). Social constructionists have, of course, provided their rejoinders (cf. Atkinson 1995; Nicolson and McLaughlin 1987; Weinberg 2014). But despite long labors at this particular coalface, the field of medical sociology remains demonstratively ambivalent about how best to conceptualize the epistemology and ontology of disease, or the relationship between the two.

I argue in what follows that this ambivalence can be considerably clarified and reduced by adopting the ethnomethodologically informed distinction between the *topics* of sociological analyses and the *resources* used to implement them (Garfinkel 1960; Garfinkel and Sacks 1970; Lynch 1993; Sacks 1963; Zimmerman and Pollner 1970). Sociological ambivalence concerning the epistemology and ontology of disease is precisely an ambivalence concerning whether disease categories are best understood as topics of, or as resources for, medical sociological research. The first section critically reviews the topic/resource debate in ethnomethodology, demonstrating its wider relevance to contemporary social theory and to the epistemology and ontology of disease. More specifically, it shows how this debate evolved from an early preoccupation with purifying sociology of common sense, or members' assumptions, to a radically empiricist attention to members' own ontologies and practices and then, most recently, into a more cautious and refined enquiry into the precise relationships that obtain between our own and our research subjects' epistemologies and ontologies in particular cases. The second section further elaborates upon this connection through a more detailed discussion of sociological debates directly focused on the epistemology and ontology of disease. The article concludes by demonstrating how framing the epistemology and ontology of disease in terms of the topics and resources of medical sociological analyses serves to clarify the work of thinking sociologically about disease and helps overcome protracted theoretical challenges that have long vexed medical sociological research.

THE TOPIC/RESOURCE DEBATE IN ETHNOMETHODOLOGY

Largely via Alfred Schutz, ethnomethodology took some of its earliest initiatives from Edmund Husserl (cf. Heritage 1984; Lynch 1993). Husserl's (cf. 1970:310–11) project was to produce a "formal ontology" that grounded science in a universalistic account of human experience by showing how the "natural attitude" inhabited by both lay actors and scientists gives people the sense of a phenomenal world stable enough to sustain systematic scientific theorizing. While neither Schutz nor Harold Garfinkel embraced Husserl's transcendental phenomenology, both did adopt his principled and categorical distinction between the analyst's ideal *theoretical* attitude and the *natural* attitude of what Garfinkel would come to call "members."[1] This distinction was reflected in the contrast ethnomethodologists drew between analytic topics and resources.

Diagnosing sociology's relative neglect of the methods and substance of common sense reasoning, Garfinkel ([1967] 1984:75) wrote, "The 'rediscovery' of common

sense is possible perhaps because professional sociologists, like members, have had too much to do with common sense knowledge of social structures as both a topic and a resource for their inquiries and not enough to do with it only and exclusively as sociology's programmatic topic." Here Garfinkel complains that sociologists have too often tacitly incorporated members' common sense knowledge of social structures as *resources* into their own analytic methods of study, thereby rendering common sense knowledge comparatively less available to sociological inspection *only and exclusively as sociology's programmatic topic*.[2] This critical orientation to sociology's reliance on common sense reasoning, or what Michael Lynch (1991:84) called a "remedial register," is evident in many of ethnomethodology's earliest canonical texts. Harvey Sacks (1963) and Aaron Cicourel (1964), for example, both argued the social sciences were stunted by their failure to fully distinguish their own methods of reasoning from common sense and offered advice on how this could be avoided. In one of the more sustained critiques of the reliance on common sense, Zimmerman and Pollner (1970:82) wrote,

> Sociology's acceptance of the lay member's formulation of … sociology's topical concerns … makes sociology into an eminently *folk discipline* deprived of any prospect or hope of making fundamental structures of folk activity a phenomenon. Insofar as the social structures are treated as a given rather than as an accomplishment, one is subscribing to a lay inquirer's version of those structures.

These early texts traded on the empirically demonstrable claim that by relying on common sense assumptions as resources, mainstream sociologists had failed to live up to their own aspirations to produce theories bearing a historically and culturally invariant correspondence with social reality. While forthright in their claims that their mainstream colleagues had failed to live up to their own self-imposed standards of literality and universality, early ethnomethodologists were usually more circumspect as to whether they themselves aspired to fulfill such standards or whether such standards could be fulfilled even in principle. Nonetheless, the professional pressures to interest their mainstream colleagues in phenomenologically inspired research no doubt encouraged use of the remedial register (Lynch 1991:84). By adopting this register, Garfinkel, Sacks, Cicourel, and Zimmerman and Pollner at the very least implied ethnomethodology might help sociologists approach such a theoretical literality and universality by unearthing and eliminating unexamined "folk" assumptions from their research.

As will be discussed in further detail below, a remedial register is also explicit in Pierre Bourdieu's more recently formulated sociology of science when he asks: "How is it possible for a historical activity, such as scientific activity, to produce trans-historical truths, independent of history, detached from all bonds with both place and time and therefore eternally and universally valid?" (Bourdieu 2004:1). Similar to early ethnomethodology, Bourdieu's answer was that by orchestrating a thorough "epistemological break" with common sense and other forms of taken for granted knowledge, social scientists can systematically incentivize and empower

those among themselves with the greatest capacity to identify the unexamined habitual assumptions corrupting their own and one another's research. This then "leaves less and less room for the unthought assumptions of doxa" (Bourdieu 1975:33). It is, then, for Bourdieu, reflexive scientific competition that dissolves the naïve objectivism of the unreflexive scientist and progressively eliminates all culturally relative assumptions. As Lynch (2000:31) highlights, though, by categorically exalting his own brand of reflexive sociology above all other forms of knowledge, Bourdieu's formulation "can lead to conflict when applied to practices that do not regard themselves as naïve." As will be shown, such conflict has become a, perhaps the, main sticking point for those of us committed to overcoming the antinomy between sociological and biomedical epistemologies and ontologies of disease.

Most ethnomethodologists themselves eventually abandoned the remedial register (cf. Garfinkel and Sacks 1970). With the later Wittgenstein they accepted that the effort to exhaustively specify one's "unthought assumptions" cannot yield Bourdieu's "eternally and universally valid" truths, but results only in infinite regress. The difficulty is that this effort is itself inevitably informed by its own unthought habitual assumptions which then require a further meta-reflexive turn which is also informed by new unthought assumptions ad infinitum. Instead of seeking to cleanse sociology of common sense resources, most ethnomethodologists have sought, seemingly more modestly, to analyze as topics the manifest anatomy of the collective reasoning practices observable across various domains of social life (including biomedicine). This effort, then, is less focused on purifying and thereby privileging the distinctive epistemologies and ontologies of sociological or ethnomethodological science than on faithfully rendering the epistemologies and ontologies collectively accomplished in practice by research subjects themselves.

While on its face this initiative appears opposed to the goal of exalting one's own epistemological privilege, closer examination reveals among some of ethnomethodology's more prominent practitioners what seems to be a radically empiricist effort to do just that (Pollner 1991:373–74). Whereas early admonitions to avoid confusing topics with resources encouraged efforts to epistemologically purify and exalt sociological theory, more recent research often seeks to downplay the distinctiveness of ethnomethodology's theoretical resources, perspectives, and objectives in favor of a radically empiricist observational science (cf. Berard 2005; Pollner 1991; Schegloff 1999). Contrary, then, to Bourdieu's effort to reflexively purify sociological theory of common sense biases, or what he called "doxa," many ethnomethodologists have seemed to adopt what Garfinkel and Sacks (1970:345–46) called an "ethnomethodological indifference" to sociological theory, suggesting, at least implicitly, that we might simply allow the empirical social world to reveal itself directly, unmediated by our own theoretical resources, interests, and objectives. Abjuring self-conscious regard to one's own theoretical resources as such, concern shifted to treating all forms of knowledge only and exclusively as topics of empirical investigation (Pollner 1991). Writing from ethnomethodology's, in many ways,

kindred tradition of conversation analysis, Emanuel Schegloff (1999:581–82), for example, writes,

> In writing that "'mere description' is not viable in ordinary discourse," I meant precisely to imply a contrast with disciplined empirical inquiry, in which it *is* viable, or can be. It is, to be sure, true that the "indefinitely expandable set of noticings" is a generic characteristic of the world addressed by disciplined naturalistic inquiry, but its import is not the same there as it is in ordinary discourse. In ordinary discourse, "correctness" by itself is not adequate grounds for noticing. In research inquiry, it is. That is what makes such inquiry a distinctive domain of activity. "Mere description" is exactly what basic inquiry aims for — to the recurrent chagrin of those who insist on its practical relevance or payoff.

But to argue that what Schegloff here calls "disciplined naturalistic inquiry" can be categorically distinguished from "ordinary discourse" reifies generic, essentialist, and empirically empty definitional contrasts between scientific research and mundane practice and, indeed, pure and applied science. This systematically blinds us to the vast diversity of ways in which disciplined naturalistic inquiry, including ethnomethodological inquiry, is both embedded in and accomplished through broader social arrangements that both influence and are influenced by it (Dennis 2019; Jasanoff 2011; Longino 2002; Ostermann and Kitzinger 2012; Rouse 2002). When the ethnomethodologist Tim Berard (2005:7) writes of "disinterested description" as "a variety of description which is not politically or morally driven, but rather driven by scholarly concerns, according to which social inquiry should be governed by principles of empiricism and logic rather than ideology" he also performs just such a reification. But as Lena Jayyusi (1991:247) writes, "Just as one cannot get out of language to talk about language … so one cannot get out of the moral order in order to talk about the moral order. What does this mean for the analyst? It means that she/he uses her/his moral membership, her/his knowledge of the mundane organization of the practico-moral order as a resource, even as she/he turns it into a topic." Contra Schegloff above, Ostermann and Kitzinger (2012) also convincingly argue the growth of conversation analysis has produced a proliferation of topics and debates that do not readily yield to the distinction between science and ordinary discourse, or pure and applied science. Instead these debates and/or practical agendas are often demonstrably comprised of a mélange of objectives simultaneously scientific, political, cultural, medical, and otherwise.

In short, the advocacy of "mere description" mistakenly presumes access to a value free Archimedean observational standpoint (cf. Daston 2008) and what John Dewey called a spectator theory of knowledge that dramatically distorts the empirical realities of knowledge production. Moreover, as will be further argued below, it is also methodologically confining insofar as it systematically elides from our attention: (1) the contingent influences history has exerted on the specific *topics*, or ontological structures and processes, we investigate such that they appear sufficiently identifiable and enduring to sustain systematic empirical investigation, and (2) the contingent influences history has exerted on the specific analytic *resources*

and practices through which we ourselves seek to study the world. While attending to these influences will certainly fail to provide Bourdieu's "eternally and universally valid" truths, it will provide indispensable materials with which to formulate particular (and empirically grounded) rather than universal (and empirically empty) distinctions, and demonstrations of contact points, between our own objectives, perspectives, and topics as sociologists and those of our research subjects. And, as I will show, by thereby allowing us to dissolve the abstract, universalistic, and empirically vacuous categorical antinomy between biomedical and sociological epistemologies and ontologies such attention can also significantly reduce the reigning ambiguity and analytic ambivalence among medical sociologists concerning the epistemology and ontology of disease.

THE EPISTEMOLOGY AND ONTOLOGY OF DISEASE IN MEDICAL SOCIOLOGY

As noted earlier, until the late twentieth century sociologists assumed they were unqualified to study the epistemology and ontology of disease. Instead of focusing on disease itself, medical sociologists predicated much of their work on the distinction between disease and illness (Conrad and Barker 2010; Eisenberg 1977). This distinction relied for its original sense on an a priori metaphysical resolution to divide the world into two mutually exclusive and pre-given ontological wholes — nature and culture. The ontology of disease was assigned to the natural side of this partition, and was understood as an objective, mind-independent, and culturally invariant reality discoverable by natural scientific methods. Conversely, illness was the subjective experience of disease which was, of course, colored by the idiosyncratic and evolving interpretive resources and social structural contexts through which all non-scientific human contact with nature is mediated. Subject, as it was, to the vicissitudes of history and interaction, illness fell within the province of the social sciences.

Since the groundbreaking research of pioneers like Anselm Strauss and Barney Glaser (1975), the illness experience perspective has become a mainstay of medical sociology (cf. Charmaz 1993; Conrad 1987; Pierret 2003; Rier 2010). While proliferating new sociological (as opposed to biomedical) *topics*, the illness experience literature has not uniformly abandoned reliance on biomedical knowledge as a pool of theoretical *resources* for the conduct of these analyses. Hence, for example, the essays in Strauss and Glaser (1975) tended to address the distinctive burdens imposed by biomedically specified disease conditions including rheumatoid arthritis, ulcerative colitis, diabetes, emphysema, and renal failure. Likewise the essays in Anderson and Bury (1988) consider biomedically specified conditions including multiple sclerosis, Parkinson's disease, arthritis, and others. And as Pierret (2003:5) notes in her review of the literature, the "huge majority of these publications report on the experience of a specific illness: epilepsy, Alzheimer's, rheumatism, polio, heart disease, cancer, HIV-infection, etc." These studies do not normally treat as *topics* the genesis of these diagnostic categories as members' concepts (or members' uses of them),

but instead uncritically adopt these members' categories as *resources* with which to denote biological (i.e., ontological) causes of what Bury (cf. 1982) has called "biographical disruption." But as ethnomethodology has long highlighted, the conflation of members' (diagnostic) categories with ontological causes of (illness) experiences masks the fact that these categories are abstract typifications, the production and application of which are invariably situation specific practical accomplishments (cf. Maynard and Turowetz 2019; Rosenberg 2002).

That said, some of the illness experience literature has fruitfully addressed the topic of members' uses of diagnostic categories as resources by showing the uncertainties, complications, limitations, and distortions that arise in applying these categories to the unique problems of particular individuals. Bury (1982), for example, described people's tendency to entertain a range of medical and nonmedical hypotheses regarding the source(s) of their problems before seeking medical help. And even after enlisting medical help, unequivocal diagnoses are often elusive. In their classic study of epilepsy experiences, Schneider and Conrad (1983:60) write,

> A seizure disorder like epilepsy is not a clear-cut disease with uniform signs and symptoms. The diagnosis is the product of clinical medical judgment … At times, doctors have difficulty making a diagnosis or finding a satisfactory treatment, so they try different hypotheses and solutions.

Moving beyond the illness experience, Renee Fox (cf. 2000) elevated the topic of uncertainty by demonstrating its ubiquity throughout modern biomedical practice. She and others have contributed to a picture of rampant over-compensation for medical uncertainty rising into the hubris of hyper-certainty. Donald Light (1979:320), for example, argued that medical "professionals run the danger of gaining too much control over the uncertainties of their work by becoming insensitive to complexities in diagnosis, treatment, and client relations" (see also Katz 1984:165–206; Timmermans and Berg 2003). Relatedly, Nicholas Christakis (1999:4) explained physicians' widespread tendency to avoid prognoses as "partly a consequence of the contemporary dominance of an ontological view of disease — a view in which disease is seen as generic and generally independent of its expression in an individual … Once a diagnosis is made and effective therapy initiated, the clinical course of a disease is often presumed to be relatively fixed — the same for everyone." By this view, distinguishing prognosis from diagnosis and therapy only erodes that faith, thereby reintroducing clinical uncertainty. The medical uncertainty literature ubiquitously reveals a rampant reluctance among clinicians to critically consider the epistemological fallibility of biomedicine's conceptual and clinical resources or the ambiguity, contingency, and particularity of the ontological topics to which they are applied.

In an important step forward in the medical uncertainty literature, Paul Atkinson (1995:117) highlighted the inconsistency of uncertainty itself:

> … It is vital that we be sensitive to contexts and local variations in examining the expression of uncertainty … It is not enough to account for these things in terms of generic and pervasive features of medical culture. There is a need for detailed

> examination of how medical practitioners, students, scientists and others express and discuss their information, how they voice their opinion, and how they claim particular warrants for the knowledge and interpretations they endorse.

Atkinson's insights here dovetail with a broad range of research that demonstrates how biomedical practice is a heterogeneous and widely distributed set of activities conducted under discontinuous institutional circumstances (cf. Fujimura 1992; Gubrium 1986; Haraway 1991; Mol 2002). These institutionally contingent activities are not necessarily congruent, but may be antagonistic, or, more commonly, simply mutually irrelevant. Since the late twentieth century we have also witnessed a growing respect for knowledge produced outside the confines of the "biomedical model." Beyond alternative medicines, patients are increasingly organizing into support groups that often provide therapeutic services themselves (cf. Brown and Zavestoski 2005). While some of these groups avoid trespassing on the traditionally biomedical province of disease diagnosis, many do not (cf. Weinberg 2005, 2013). These developments are putting increasing pressure on social researchers to forsake the image of the sovereign biomedical diagnostician unearthing the bedrock of invariant ontological structures and processes underlying our various forms of unwellness and to appreciate the diversity, evolution, and proliferation of both biomedical and other sorts of diagnostic systems and categories (Jutel 2009, 2011; Nissen and Risor 2018; Smith-Morris 2016). Most of this research focuses on the local epistemological warrants for health related knowledge rather than the ontology of disease as such. However, because it highlights the contingency, discontinuity, provisionality, and uncertainty of such knowledge, this work presents an at least implicit provocation to reconsider the orthodox biomedical ontology of disease.

Defending a critical realist approach, Simon Williams (2003:52) has properly insisted that "what we know and how we know it (epistemology) … should not be confused with what there is to know (ontology)," and "[d]isease labels, one might say, *describe* but do not *constitute* disease." Williams is certainly correct to distinguish disease categories as conceptual resources from the topics — ontological sources of suffering and disability — to which they are applied. Unfortunately, however, his critical realist construal of this distinction completely severs them from one another and — in line with Kant's classic discussion of the "thing in itself" — ultimately renders the ontologically "real" epistemologically unknowable. For Williams, medical topics (the ontologically real sources of suffering and disability) are comprised of "intransitive objects … which exist and act independently" of our knowledge of them (Williams 1999:808). Like other critical realists, he is careful enough to allow that the putative "properties and powers" of these unknown but real objects (they must be putative because, by definition, they exist and act independently of any actual knowledge of them) are only "relatively enduring." This frees "critical realism from the charge of over-determinism in which structures can never be changed" (1999:808). Regrettably, though, such generic philosophical postulates provide us no information about: (1) the properties and powers of specific real objects (diseases or otherwise); (2) which real objects are causally active in any given domain of

empirical research; (3) how enduring or ephemeral specific objects are; or, (4) which objects should be understood as elemental and which compounds of more elemental objects. In short, they provide no conceptual resources of any epistemological value to any particular, or actual, scientific investigation of particular medical topics.

Moreover, Williams (1999:814) is not content to philosophically posit the fundamental ontological properties and powers (e.g., the essential intransitivity and transfactuality) of real objects without empirical evidence. He wants also to philosophically assign these imagined and empirically elusive objects to mutually exclusive sets such that "the biological" and "the social" might be categorically distinguished not just as conceptual resources with which particular scientific topics might be productively framed, but as "real ontological strata," indeed, irreducible ontological wholes (Williams 1999:814).[3] Perhaps most problematic for present purposes is the notion that diseases, if they are ontologically real, must exist entirely independently of knowledge and perception. This position is starkly incompatible with the increasingly undeniable assertion that disease categories are intrinsically value-laden in the sense that they denote ontological referents that invariably cause *harm* (cf. Ereshefski 2009; Murphy 2009; Wakefield 1992). Ontologically speaking, it is plainly uncontroversial to argue that harmful agents exist. How one might speak of harms as culturally invariant, value-neutral features of a mind-independent reality is quite another matter — a matter about which critical realists have remained conspicuously silent. We are instead simply instructed to uncritically take biomedicine's current formulations of disease at face value as valid renderings of the enduring ontological realities they purport to represent.

Williams is by no means alone in his efforts to foster a greater deference to biomedical knowledge in medical sociology (Williams 2006). For example, though coming from a very different social theoretical vantage point, Timmermans and Haas' (2008) charter for a sociology of disease also calls for a greater and less critical deference to biomedical knowledge. These essays present two of the most eloquent voices in a much larger chorus in medical sociology that criticizes social constructionism for a social reductionism that discounts the reality of disease and disability (cf. Bury 1986; Shakespeare 2006; Turner 2004). This literature is quite properly critical of a radical social reductionism that renders diseases as wholly sociogenic — indeed, as mere labels, myths, discourses, narratives, or roles. Unfortunately, though, it systematically omits consideration of the core challenge social constructionism raises — that of reconciling claims that diseases are historically invariant and universal biological realities with the manifestly multiple, evolving, contingent, and sometimes conflicting standards that govern the objective empirical identification and management of diseases in actual biomedical practice (Timmermans and Berg 2003). It thereby overlooks the fundamental and indisputable scientific facts that: (1) the biological regard for diseases is heterogeneous and evolving, and (2) biomedicine does not possess a monopoly on the epistemology and ontology of disease (e.g., Aryuvedic Medicine, Traditional Chinese Medicine). *Moreover, it also neglects the more specific questions of how various scientific and*

non-scientific cohorts collectively assess the credibility of ontological claims — that is, the local relationships people draw between the epistemology and ontology of disease across diverse cultural and institutional contexts. In the next section, taking my cues from the enduring ethnomethodological interests in precisely such topics and Ludwik Fleck's seminal work in the sociology of biomedical science, I argue that the collectively orchestrated perceptual habits constitutive of what Garfinkel once called the natural attitude, rather than mind-independent realities, provide these cohorts with the ontological topics to which their various conceptual resources are applied. Using Pierre Bourdieu's profoundly influential concept of habitus to demonstrate the pitfalls of radically reductionist sociological explanations of our perceptual habits, I suggest a way forward in the Fleck inspired observation that perceptual habits are *objectively* found across diverse cohorts to have been influenced by a wide range of both social and other (including biological) forms of empirically identified structure (cf. Latour 2004).

Symptomatic of medical sociology's enduring ambivalence as to the epistemology and ontology of disease, both Williams and Timmermans and Haas discuss Annemarie Mol's modern classic, *The Body Multiple*, in largely approving terms while at the same time making biomedically reductionist statements starkly incompatible with Mol's core thesis that the ontology of disease is a feature rather than a mind-independent cause or parameter of socially contingent forms of shared practice.[4] In both cases, otherwise discerning commentaries are compromised by an over-simplification of the diverse relationships that obtain between sociological and biomedical epistemologies and ontologies of disease that unnecessarily renders them mutually exclusive and incommensurable. Winner of both the 2004 Sociology of Health and Illness Book Prize and Ludwik Fleck Award, *The Body Multiple* was described by Arthur Frank (2003:532) in his *American Journal of Sociology* review as "my nominee for defining medical sociology in the 21st century." Mol targets what she argues is the anachronistic distinction between disease as socially invariant objective reality and illness as socially variable subjective experience by contrasting how two forms of medical practice enact what is ostensibly one disease in very different ways. By closely attending to the practices by which clinicians and pathologists enact atherosclerosis, she demonstrates that indeed they are working not only with different epistemological frames or practical agendas but with different ontological *things*. As she (2002:35) writes,

> The practices of enacting clinical atherosclerosis and pathological atherosclerosis *exclude* one another. The first requires a patient who complains about pain in his legs. And the second requires a cross section of an artery visible under the microscope. These exigencies are incompatible, at least: they cannot be realized simultaneously. This is not a question of words that prove difficult to translate from one department to the other. Surgeons and pathologists who talk with one another tend to understand each other very well. It is not a question of looking from different perspectives either. Surgeons know how to look through microscopes and pathologists have learned to speak with living patients. The incompatibility is a practical matter.

However, despite their differences, Mol deftly demonstrates how clinicians and pathologists practically link their respective work objects through situated collaborations with one another. Clinical findings of atherosclerosis are sometimes confirmed in the pathology lab and vice versa, but not always. The real world is messier than that. Attending to the details of medical practice rather than the abstractions of medical knowledge, Mol shows that practitioners inhabit not a universe — a unified domain of objects behaving together as a singular system — but a multiverse comprised of both familiar and unfamiliar objects that often resist subsumption into a singular system of knowledge. Moreover, even when we succeed in securing it, medical knowledge is itself inevitably subject to anomalous findings, revision, and outright falsification. The ontologically real, for Mol, is very obviously distinct from epistemology but, pace critical realism, is neither imperceptible nor equated with abstract and empirically empty philosophical generalizations. Mol convincingly articulates a dynamic middle road between the a priori ontologies of biological and social realism. Neither biological nor social structures are cast as mind-independent realities lying beneath people's subjective experiences and practices. Instead, both their reality and causal relevance are cast as intrinsically emergent in and through hospital practices.

While Mol's contributions are undoubtably profoundly important, there do remain issues pertaining to her analysis that merit further attention. The first to consider is the uncontested status of atherosclerosis in her study as an empirically identifiable medical phenomenon. As Malcolm Ashmore (2005:829) notes, "Atherosclerosis is neither novel nor uncertain. Nothing in Mol's ethnography suggests that its reality and truth are anywhere contested … This then is Mol's major contribution: to show that the assumption of stability … in 'finished science' is inappropriate … the epistemic labour of coordination, distribution and inclusion continues." Yet, however, the absence from Mol's narrative of the specific history that yielded the "finished science" of atherosclerosis leaves unexplained how clinicians and pathologists acquired the *resources* with which they now forge and provisionally stabilize bodies multiple in practice and how the use of these resources became comparatively routinized. Absent a social historical explanation, orthodox biological realists may be all too prone to insist these resources were somehow furnished by the inevitably stable and putatively mind-independent biological body singular. Williams (2006:21–2), for example, doubts whether Mol's analysis sufficiently provides for how "the body and disease 'hang together' or 'cohere' through practice" and asks "Does Mol's emphasis on the enacted nature of reality in practice … leave adequate room … for the abiding nature of reality between or over and above any such practices?" An historical genealogy of her ethnographic research settings could fortify Mol's argument against devoted biological realists like Williams by empirically demonstrating specifically how the reality of atherosclerosis was *historically* rendered a matter of medical consensus through emergent social practice rather than by an historically invariant and putatively mind-independent biological body singular.

Second, much like Schegloff's aspiration to "mere description" above, throughout *The Body Multiple* and elsewhere Mol seems to suggest the analyst can delegate *all* of the ontological work to her research subjects and, it follows, produce an empirical study without any distinctive ontological commitments of its own. As she explains, "the attempt of [*The Body Multiple*] was to shift the doing of ontologies from me … to the practices that formed my object of research, hospital practices" (Woolgar et al. 2008:6). Treating diagnosis as an ethnographic topic rather than resource, she abjures any invocation of a singular biological atherosclerosis underlying the myriad practices through which it is variously enacted. On the other hand, and this must be fully appreciated, Mol also asserts her own positive ontological claim that "atherosclerosis is more than one but less than many" (Mol 2002:55). Quite obviously, this is an ethnographically informed claim about what the disease really is that is not identical with any particular instance of its practical enactment in the hospital and, for that matter, with which her research subjects in the hospital might very well disagree.[5]

By going beyond treating the diagnosis "atherosclerosis" only as a topic — a concept used by her research subjects to identify particular clinical and pathological work objects — to treating it as a resource — a concept she herself uses to identify a general or recurrent ethnographic work object that is more than one but less than many — Mol appears to depart from her claim to have "shift[ed] the doing of ontologies from me … the analyst … to the practices that formed my object of research, hospital practices" (Woolgar et al. 2008:6). Mol puts a conceptual resource members routinely make meaningful exclusively through hospital practices to novel philosophical and social scientific uses. Because she cannot have done otherwise without becoming a clinician or pathologist, this is by no means a flaw in her analysis. But by glossing over this transition from medical usages as topics to a social scientific usage of the diagnostic category as an analytic resource of her own, she camouflages her own distinctive ontological construal of atherosclerosis as no more than a transparent representation of members' practices, encouraging, as did Schegloff in his advocacy of "mere description," a positivist erasure of herself as analyst.[6] By refraining from more explicitly distinguishing her own ontological claims and interests from those of members, Mol also runs the risk of depreciating the importance of investigating the distinctive socio-historical processes that institutionalized not only the particular differences and potential meeting points between clinical and lab practices but those that institutionalized the particular differences and potential meeting points between her own ethnographic ontological work and the ontological work of those she studied in the lab and clinic.

A PROPOSAL FOR MOVING FORWARD

What is needed to move forward in debates concerning the epistemology and ontology of disease in medical sociology, then, is an approach that avoids radically isolating them from one another by an untraversable metaphysical chasm (as do critical

realists like Williams) but does not conflate ontological topics and epistemological resources by means of positivist presumptions to "mere description." Following the philosopher of science Willard van Orman Quine, we must relinquish the idea that philosophical generalizations of the sort produced by critical realists might somehow legitimate or steer science from a position external to it (cf. Rouse 2002). Philosophy simply does not occupy an Archimedean standpoint from which to posit either universal ontological structures or universal epistemological standards for the legitimation of knowledge. But neither is there an Archimedean standpoint from which one might engage in the kind of "mere description" advocated by Schegloff and, seemingly, Mol. As Garfinkel taught long ago, we have, then, no alternative to intrinsically *provisional* substantive analyses of the emergent processes through which our theoretical resources and the ontological topics to which they are applied are practically realized (that is, provisionally made real) and related to one another both among those we study and among ourselves as analysts.

Beyond ethnomethodologically embracing the intrinsically practical and situated provisionality of epistemological resources and ontological topics in this way we must also more emphatically topicalize the perceptual habits Garfinkel held constitutive of the natural attitude in place of the metaphysical chasm that Kantians, including critical realists, have posited between a putatively mind-independent objective reality and subjective, including scientific, perception and agency. Habit indispensably furnishes a pre-theoretical empirical world of perceptible ontological topics irreducible to the analytic resources we use to account for them. It is therefore prospectively useful to discursive knowledge production in a way that a mind-independent world, because it is imperceptible, can never be.[7] As the acclaimed historian of science Lorraine Daston (2008:99) has argued, "It is habit that makes perception of a world possible … The novice sees only blurs and blobs under the microscope; experience and training are required in order to make sense of this visual chaos, in order to be able to see *things*." Citing the biologist Ludwik Fleck's groundbreaking contributions to our understanding of the ontology of disease, Daston (2008:100) writes,

> For Fleck, learning to see like a scientist was a matter of accumulated experience—not only of an individual but of a well trained collective. The fault line in epistemology did not run between subjects and objects, the great Kantian divide, but, rather, between inexperience and experience. Unlike the neo-Kantians, who worried about how the subjective mind could know the objective world, Fleck was concerned with how perception forged stable kinds out of confused sensations … Another way of putting this contrast is to say that Fleck was more interested in ontology than in epistemology.[8]

But to argue for the importance of collectively orchestrated habit formation to an ontology relevant for scientific research is not yet to articulate a satisfying theoretical model of such a thing. Pierre Bourdieu is the social theorist who most systematically expounded the centrality of habit to social life and perception (cf. Bourdieu 1990). As a sociologist, Bourdieu was always eager to highlight the profound extent to

which habits, or habitus, embody the specifically social determinants of our lives. Though there is nothing in his formulation that demands the habitus be construed as exclusively sociogenic, Bourdieu has been occasionally taken to task for asserting a sociologically reductionist cosmology wherein structural causes of the habitus that are not social, including putatively biological structural causes, are merely residual stipulations rather than fully integrated aspects of his analyses (cf. Dreyfus and Rabinow 1993; Latour 2005; Sismondo 2011).[9] While this problem is solvable, its solution requires a softening of Bourdieu's methodological insistence that our theoretical resources be categorically distinguished from our ontological topics or, as he put it, that we execute a categorical epistemological "break" between our own knowledge and that of our research subjects (cf. Bourdieu 1990).

Such a softening would shift Bourdieu's position considerably closer to so-called post-humanists like Donna Haraway, Karin Knorr-Cetina, Bruno Latour, and Annemarie Mol. Like Fleck, these researchers favor a more porous sociology wherein we might sometimes treat our research subjects' practices and perceptions, including their discursive testimony, not only as *topics* for reductive sociological explanation (e.g., as invariably symptomatic of their locations in Bourdieuian social fields which they themselves inevitably misrecognize[10]) but as provisionally epistemologically legitimate, or provisionally valid, theoretical *resources* in their own right. This would allow for the possibility that people's activities and experiences are sometimes caused not only by their interactions with structures or mechanisms identified by Bourdieuian sociologists but with structures or mechanisms identified by pathologists, clinicians, biologists, psychologists, lay actors, and others. It was Bourdieu's vigorous opposition to such a porous sociology that Lynch (2000:31) described as a source of "conflict when applied to practices that do not regard themselves as naïve." Sadly, Bourdieu is not alone in promoting this kind of radically sociologically reductionist approach. Among other liabilities, this approach decisively precludes even the most provisional and circumscribed reconciliations (or fruitful disagreements) between sociological and alternative (including biomedical) epistemologies and ontologies of disease.

The medical sociology of disease is now too often seemingly stuck between the Scylla and Charybdis of a priori sociological and biological reductionisms. One must either, it seems, with Bourdieu, explain biomedical practices, including the biomedical identification and engagement with disease, with exclusively socio-historical rather than biomedical, or any other presumably asocial, theoretical resources; or, like the critical realists, reify biomedical topics by philosophically projecting them into a putative ontological domain beyond the scope of empirical evidence, knowledge, and social history. We are thereby forced to choose between two equally valid scientific claims: (1) the sociological claim that disease categories are conceptual *resources* devised and applied in accord with culturally specific and socially evolving standards and training and (2) the biomedical claim that the *topics* — ontological structures and processes — empirically denoted with these categories are often manifestly asocial rather than social causes of suffering and disability. We need to

accommodate both. Second, but equally perniciously, by philosophically predefining biology and society (or "layers" within them) as mind-independent and mutually exclusive "ontological strata" we deprive scientists, clinicians, and other prospective experts of the authority to specify and revise their inevitably provisional regard for the empirically objective nature of these domains, the objects that populate them, and the sorts of relationships that might be discovered between them (Benton 1991; Rouse 2002). This a priori conceptual deadlock has long thwarted progress in the medical sociology of disease.

Let me now finish this section with a brief empirical case study of how to transcend the conceptual deadlock between a priori biological and sociological reductionisms. Consider the following ethnographic data excerpt drawn from fieldwork I conducted in a therapeutic community for people dually diagnosed with an addiction and major mental illness (cf. Weinberg 2005). For a variety of reasons, residents of this therapeutic community were often found to both feign and conceal ill health. Given the ever-present possibility that people were dissembling, prima facie evidence of health and pathology was never taken completely for granted. This excerpt is taken from a staff meeting wherein the authenticity of a particular resident's recent epileptic seizures is discussed.[11] Jane, a counselor, asked,

> "What are we going to do with Ruby?" There were a lot of frowns and shaking heads following this question. Eve, another counselor, replied, "I don't know. What *can* we do is the question."
>
> Jane asked, "Are we gonna have to let her go?"
>
> Eve answered, "I think we might have to. This program is just not equipped to deal with someone with her kinds of problems. Apparently she had another seizure last night … "
>
> John, another counselor, asked, "What about those seizures? What do you think those are about?" There was more shaking of heads then John followed up on his own question, "I mean do we know for sure if they're even real?"
>
> Jane said, "Yeah, some of the residents said they thought she was faking them … "
>
> Eve said, "Really? Why would she do that?
>
> John said, "Who knows? It could be anything … "
>
> Eve asked, "Why did the residents think she was faking?"
>
> Jane answered, "I think you can more or less tell with seizures if they're real or not. Tracy (a resident) said her eyes rolled back in her head and she started to shake slowly and then it got worse. But she, like, got down on her side and started shaking around and then got up looking all disoriented." Jane gave a look suggesting she thought it could be an act.

> John said, "I think they might be fake. I mean she's had like two or three since she got here. That's a bit much I think."

This conversation begins with pessimistic reflections on the social model program's clinical limitations in the face of Ruby's evident neurological affliction and the possible conclusion that they may need to refer her to a more appropriate clinical caregiver more medically equipped to deal with "her kinds of problems." The exchange is clearly focused on the ontological *topic* of Ruby's evident seizures as empirically furnished by members' common sense perceptions (rather than the mind independent reality) of Ruby's behavior. However, as the conversation proceeds, these common sense observations are subject to a more rigorous analysis via a set of members' competencies informed simultaneously by conceptual resources drawn from (1) general biomedically informed orientations to how epilepsy *typically* presents both in terms of its behavioral qualities ("eyes rolled back in her head," "shaking," "disoriented") and its episodic quantity ("she's had like two or three since she got here. That's a bit much I think.") and (2) general sociologically informed orientations to practical action as *typically* based on reasons ("Why would she do that?" "Why did the residents think she was faking?"). This discussion, then, exhibits a hybrid transcendence of a priori biological and sociological reductionisms. The discussion of whether Ruby's seizures were "real" or "fake" was precisely a discussion of whether the ontological status of Ruby's evident seizures was biological or sociological. And, as can be seen, this discussion was intrinsically provisional and based in the first instance on the empirical evidence provided by habituated, practically embedded, perception.

Moving on from members' own practices of transcending a priori reductionisms, let us consider the methodological upshot of ethnographic materials like these for our own possibilities as sociologists to transcend the antinomy between sociological and biological reductionisms. As has been noted, were we to take Bourdieu's notion of a decisive epistemological break seriously, we would seek to analyze this discussion through the purely sociological prism provided by the conceptual triad of habitus, field, and capital. The debate over the ontological status of Ruby's putative seizures would be explained in terms of the various forms of capital at work in this particular field, and the specific sorts of investments these actors had come to dispositionally embody in the form of their habitus. It is these, and only these, that an orthodox Bourdieuian would say give empirical form to the topics and resources members bring to the discussion. By these lights, only social structures appear in our explanation. The debate over the ontological reality of Ruby's seizures is thereby reduced to a clash of invariably sociogenic schemata of perception and appreciation.

But as Williams (2006:9) quite properly complains, radical forms of sociological reductionism like Bourdieu's cannot provide for the myriad ways our specifically biological "[b]odies surprise us, … betray us in all sorts of ways that render our constructions of them problematic." For Williams the body, then, is not only Bourdieu's sociologically identifiable site for the incorporation of historically specific

forms of capital. It is also the biologically identifiable site of pathology, dysfunction, and death. More specifically, Williams insists bodily surprises reveal how the mind-independent ontological domain he identifies as the biological intrudes upon, constrains and, indeed, structures the processes Bourdieu would have us understand as entirely sociogenic. While I very much agree that biology does, on occasion, furnish scientifically and/or clinically satisfying explanations for such surprises (like seizures), I hesitate to endorse the proposal that it does so without exception. Do not bodies sometimes also surprise biologists? And, I might ask, does not sociology sometimes furnish scientifically and/or clinically satisfying explanations of the bodies that surprise biologists? Given that those informed by both sociological and biological conceptual resources seem to be in the same boat when it comes to bodily surprises, I am reluctant to endorse Williams' ontological equation of the surprising body with the biological body. As is suggested by the ethnographic data excerpt above, which conceptual resources — biological, sociological, or otherwise — best explain the bodily surprises, or what Bury called biographical disruptions, that variously arise in our lives is a question best answered not categorically and in the abstract but provisionally and with respect to the available evidence in any given case.

Building upon these points, another important research method that can be used to transcend biological and sociological reductionisms is reflexivity. Not only are our research endeavors guided by evidence alone, as positivists would have it. They are also guided by the particular mix of scientific, clinical, policy, cultural, political, economic, and other questions we seek to answer in any given project. Empirically informed reflexive dialogue hones our research skills by facilitating a more explicit regard for the specific nature of our shared work in all of its myriad forms and the distinctive resources and constraints that attend the specific conditions under which it is accomplished. This type of exercise systematically moves us beyond the confines of various axiomatic, or a priori, theoretical preconceptions and facilitates a much more nuanced and contingent understanding of what, in any given instance, our research objectives actually are. These might often include interdisciplinary collaborations wherein contingent discussions like the one above regarding the nature of Ruby's seizures are undertaken not only by our research subjects but among ourselves as analysts and all of those with whom we collaborate. Contra Bourdieu's categorical epistemological break, ethnographers have grown increasingly appreciative of the fact that practitioners of other research traditions and, indeed, our research subjects themselves often have a great deal to teach us. In particular, they often have much to teach us about the various types and sources of biographical disruption we all are prone to sometimes suffer.

The prospective fruitfulness of linking different research projects or facilitating dialogue across disciplinary boundaries is never a foregone conclusion. But contrary to sociological and biological reductionisms, such dialogue is plainly possible even in the absence of shared axiomatic foundations and often does prove fruitful in many different and sometimes unanticipated ways. Far less fruitful than examining

the prospective benefits of interdisciplinary dialogue in any particular instance, though, is the wholly speculative task of imagining the foundational relationship(s) that might universally hold between biology and society construed as if they were somehow wholly distinct, pre-given and internally integrated ontological wholes.

CONCLUSION

This article began by noting medical sociologists have long struggled with whether to be accomplices or critics of biomedicine. This struggle has often been cast as a choice between endorsing and critiquing the validity of biomedicine's diagnostic terms of reference. I do not mean to suggest this choice has had to be all-inclusive or indiscriminate. One can, of course, endorse some biomedical terms of reference and critique others. However, our capacity for epistemologically justifying such nuance and situated discretion has been unnecessarily undermined by an enduring and entrenched ontological antinomy between biomedically and sociologically reductionist realisms. Medical sociology has too often remained confined by the mistaken reification of the adjectives "social" and "biological" into categorically separate "ontological strata." Rather than regarding these adjectives as epistemic resources the usage and ontological referents of which are forged and historically evolve within myriad discontinuous practical contexts, we have too often encumbered our investigations with the unwarranted presupposition that these terms are anchored in enduring, mutually exclusive, and mind-independent ontological domains.

I have argued that replacing this a priori, abstract, monolithic, and empirically unsubstantiated imagery with that of situationally embedded epistemological resources and locally perceptually available ontological topics is a useful step forward (see also Dennis 2019). By reminding us that both epistemological and ontological questions are raised and addressed for local reasons with local theoretical repertoires and in accord with local standards of value, the language of topic and resource effectively deflates and replaces abstract metaphysical speculation with empirically grounded scientific dialogue and debate. The topics that concern different scientific communities are thereby respecified not as ontologically obdurate and timeless sources of incommensurability and disciplinary isolation but as amenable to modification, cross-disciplinary collaboration, and mutually constructive dialogue and critique. In short, we replace the hopelessly abstract and unanswerable question of biology's generic relations to society or culture with more precise and far more pressing questions concerning the respective capacities of particular social, biomedical, and interdisciplinary research cohorts to effectively address the particular clinical, policy, scientific, cultural, political, economic, and other challenges that empirically demonstrably arise in our often common worlds.

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NOTES

1. Garfinkel formulated the natural attitude as the largely habituated, unreflective "background expectancies" that encourage members to take the myriad realities they inhabit for granted. His famous breaching experiments were designed precisely to draw out into the light of day the otherwise tacit and taken for granted, or habituated, features of common sense orientations to the factual or real. As will be argued later in the article, this understanding of habituated, practically embedded, perception can be further elaborated with reference to the work of Ludwik Fleck as an alternative to realist and positivist ontologies that posit a mind-independent reality beyond perception that only the work of scientific analysis can reveal.
2. One example will suffice to illustrate the basis of the complaint. When surveys require respondents to choose between the common sense categories male and female, they thereby not only ignore how this binary is variously accomplished in practice, they discourage our even contemplating its historicity or reformation.
3. In a later essay Williams (2006:18) qualifies this position by sub-dividing biology into "a complex, multi-layered domain" but because he remains committed to the critical realist definition of the real as mind-independent, intransitive, and transfactual, this does not obviate the fundamental problems of bridging the chasm between epistemology and ontology or, assuming they are real, warranting the a priori specification of these "layers" without any empirical (mind-*dependent*) knowledge of them.
4. Williams (2006:14–15), for example, promotes a critical realist ontology of disease as mind-independent in stark contrast to Mol's ontology of emergent social practice. For their part, Timmermans and Haas (2008:660, 666) write, "social scientists have become mainly interested in the experience, culture, and social structuring of illnesses while bracketing the biological bedrock of disease" and "The idea is here not to regard biomedical pathology as a determinant of the social experience but as a group of characteristics that set the parameters of social interaction." Both of these essays categorically distinguish "the biological bedrock of disease" from emergent social practice rather than construing it, like Mol, as inseparable from, and invariably realized only through, social practice. By using nouns like "bedrock" they also promote a blanket deference to biomedical knowledge as somehow more foundational than other forms of knowledge. This said, Timmermans has in more recent work exhibited a more nuanced appreciation of the intrinsic sociality of the ontology of disease that does not so obviously ontologically bifurcate "the social" from "the biological" (cf. Timmermans 2017; Timmermans and Buchbinder 2012). I should therefore emphasize that my objective in this article is not to critique Timmermans' current views on the ontology of disease but to textually demonstrate in some of his work and that of many others an enduring and pervasive ambivalence in the field of medical sociology concerning the epistemology and ontology of disease.
5. I thank Akbar Ansari for bringing this to my attention.
6. James Laidlaw (2015:184) has noted this tendency is ubiquitous in the Actor–Network literature. Arguing that causal explanation is not simply a matter of mere description but is instead accountable to the interests one, and one's audience, may have in the particulars of one's causal

explanation, he writes "Actor–Network Theorists have their own reasons (no doubt not always the same reasons …) for emphasizing the significance of non-human entities in explaining particular states of affairs. The important point, for my purposes here, is just that those interests, whatever they are, are those theorists' interests, and not necessarily those of the people … they describe."

7. As Daniel Breslau (2000:293) wryly wrote, "It is safe to say that no one has ever observed an instance of scientific knowledge constrained by an unknowable and meaningless material world."
8. Fleck's attention to the collective orchestration of perceptual habit formation predates Garfinkel's inauguration of ethnomethodology but resonates deeply with ethnomethodology's attention to the collective orchestration of the tacit, taken for granted, competences constitutive of what ethnomethodologists call membership. It should also be emphasized that by foregrounding members' regard for one another's observable competencies rather than the tacit perceptual habits these observed competencies reveal, neither Fleck nor ethnomethodologists need to view different members' tacit competences as identical to one another. While it may be sensible to endorse Garfinkel's observation that the intrinsic accountability of practical action fosters the acquisition and habituation of capacities to competently participate in shared practices, we need not assume these capacities, or habits, take identical forms (Turner 1994). These practical and perceptual habits need only to be sufficiently attuned to allow for meaningful collaboration. Disagreement over the ontological character of what we perceive under microscopes or otherwise remains, despite our differences, a form of meaningful collaboration.
9. Evidence of Bourdieu's sociological reductionism is not confined to his writings on habitus. For example, he defines the concept capital exclusively in terms of "accumulated labor" (Bourdieu 1986:241). By equating all power with capital (p. 242) and defining capital only in terms of accumulated labor, Bourdieu omits from his sociological purview any causes of personal ability and disability other than accumulated labor (e.g., genetic inheritance, trauma and infection, or any other asocial sources of personal capacity or incapacity).
10. As Dreyfus and Rabinow (1993:42–3) note, Bourdieu's insistence on "the idea that a specific illusion is required to make the system work demands that actors can *never* be right about their specific motivations." Analogous problems plague Bloor (1991), Collins and Yearley (1992), and many other sociologists of science who, like Bourdieu, seek to categorically privilege their own over all other claims to objectivity.
11. All names are pseudonyms.

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ABOUT THE CONTRIBUTOR(S)

Darin Weinberg is a Reader in the Department of Sociology, Cambridge University and a Fellow of King's College, Cambridge. His research focuses primarily on the practical purposes to which concepts of addiction, mental disorder, and learning disability are applied in various historical and contemporary contexts. His books include *Contemporary Social Constructionism: Key Themes* and *Of Others Inside: Insanity, Addiction, and Belonging in America*, both of which won the Melvin Pollner Prize, *Talk and Interaction in Social Research Methods* (edited with Paul Drew and Geoffrey Raymond), and *Qualitative Research Methods* (edited).